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SURGICAL CASES, FROM THE RECORDS OF THE CITY HOSPITAL,
BOSTON.

[Reported for the Boston Medical and Surgical Journal, by DAVID W. CHEEVER, M.D.
one of the Visiting Surgeons.]

ELEVENTH PAPER.—*Naso-pharyngeal Polypus, attached to the Basilar Process of the Occipital, and Body of the Sphenoid Bones, successfully removed by a Section, Displacement, and subsequent Re-placement and Re-union of the Superior Maxillary Bone.*

City Hospital.—Service of Dr. CHEEVER.

THE patient, a student, eighteen years old, entered the Hospital, July 20th, with the following history:—

About two and a half years ago he had profuse epistaxis, which continued twenty-four hours. During the six months next following this, he had only occasional slight attacks. At the end of that period he found that his right nostril was wholly obstructed, and he has never since been able to blow through it. He soon became aware of a growth behind that nostril, which gradually but steadily increased, until within a few weeks of the time of entrance, when it grew rapidly. There was some discharge, but it was not offensive until quite recently.

At the time of admission to the Hospital, the soft palate was found to be depressed and pushed forward until it hung at a right angle with the hard palate, and both it and the tonsil were inflamed. At the right side of the fauces a small ulcerated patch could be seen. By the finger, a tumor could be felt behind the soft palate, firm, full and lobulated, and extending farther up than the finger could be carried. Its lower lobes hung down into the throat. The whole of the upper part of the pharynx was occupied, except a small space on the left side. Nothing could be passed into the pharynx through the right nostril, but the left was clear. Hearing was imperfect in the right ear; and the respiration was mostly through the mouth. The microscopical examination of the debris of the tumor, removed by a digital examination, revealed only blood corpuscles, pus cells and fibrous tissue. There was no evidence of malignancy.

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The patient was able to take liquid or soft food only. The general health was good, with no hereditary predisposition. He was very desirous of an operation for the removal of the tumor.

There was no question that the tumor must be removed, or, before long, cause the death of the patient. The problem to be solved was as to the best method of operating. Three modes offered themselves for consideration.

1st. By ligature, or the *écraseur*, through the nares. This was impracticable, because nothing could be passed through the right nostril opposite to which the bulk of the tumor lay; also because it was not a pedunculated growth. Were it practicable, it could only cut off the growth, without eradicating it; and it would, probably, speedily recur.

2d. By section of the soft palate, of the hard palate, and removal through the mouth, with a subsequent operation for staphyloraphy. This mode, advocated and revived by Nélaton, but really as old as Hippocrates, was abandoned on account of the size and high attachments of the tumor, and the fear that room enough to manipulate could not be got through the section of the hard palate.

3d. By removing the superior maxilla—a plain and easy way, but accompanied by deformity and serious mutilation. Here was a young man with a healthy jaw and perfect teeth, and the disease wholly behind it. Could a portion of the jaw be saved? or even the whole replaced? I decided to make a horizontal section of the jaw; depress it, saving all attachments of the soft parts possible; see if the tumor could thus be reached; and, if practicable, to replace the jaw, and try to save it.

July 23d.—*Operation.* The patient was etherized during the first part of the operation, and partial etherization was renewed at intervals; he was seated in a chair, with the head on a pillow.

An incision was made from just below the inner canthus of the right eye, downwards by the side of the nose, following the naso-labial fissure, to the corner of the mouth. The inner flap was dissected up until the symphysis was exposed; and the outer, until nearly the whole of the superior maxilla was free. With a narrow-bladed saw, about three inches long, the superior maxilla was now divided transversely, about half an inch below the floor of the orbit. The blade of the saw was plunged into the zygomatic fossa, and the front and back walls of the antrum were sawn through horizontally, starting just below the articulation with the malar bone, and terminating in the anterior nares, at the lower end of the nasal bone. The ala of the nose having been lifted up, the right central incisor was next extracted. Strong bone forceps were now used to divide the alveolar process, through the socket of the right central incisor. The cut included the *alveolus only*. The hard and soft palate were not touched. The bone was now held by the palate process, palate bone, and its co-ossification with the pterygoid processes. Seizing

the alveolar process with strong tooth-forceps, the whole section of the superior maxilla was bent down and displaced into the mouth. The antrum was found to be filled by one lobe of the tumor without attachment, while the body of the tumor was attached to the upper, back part and right side of the pharynx and to the base of the sphenoid bone. The body was very firm, and the attachments were broad, covering a space two inches square. These, with considerable difficulty, were severed by scissors, introduced through the opening above the depressed section of the superior maxilla, and the base was cauterized repeatedly with strong nitric acid. The haemorrhage, which was not excessive, was thereby effectually checked. Four ligatures were applied to bleeding vessels in the course of the first incisions. With the forefinger of the right hand in the throat, and the left in the cavity above the section of the maxilla, they could be made to meet freely, and explore thoroughly the pharynx, which was now found entirely clear of obstruction.

The superior maxillary bone was now hanging with its antrum exposed; and attached by the bent, or broken hard palate, the unbroken soft palate, and the broken osseous, and unbroken muscular and vascular attachments of the pterygoid process of the sphenoid bone. On these attachments we were to rely for the restoration of the bone. The maxilla was easily pushed up into its place, and held by a silver wire passed round the left central, and right incisor teeth; and by the closing of the lower jaw. The flaps of skin were accurately approximated, and united in place by six interrupted sutures.

At the close of the operation, the pulse was 120 and of fair strength. Wine, iced milk, beef tea and opium were ordered, *pro re natâ*. 7 P.M. Pulse 132. Reaction good. Takes nourishment freely. No vomiting or pain. Urine free. R. Pulv. Doveri, grs. x. 10 P.M. Sleeping quietly. Pulse 88. Respiration free.

July 24th, A.M.—Pulse 120, good. Patient in good spirits. Face drawn a trifle to left side. There is a little swelling of the face, and some offensive odor from the clotted blood. The parts syringed with R. Tr. myrrh., ʒ i.; aqua, ʒ iv. M. P.M. Pulse 130. No pain. Takes beef-tea, milk, eggs, &c., freely. Sleeps considerably. Functions regular.

July 25th.—Pulse 96. Except some drowsiness, feels very well. Appetite good. Sutures removed, and good union found. Eye nearly closed by adjacent swelling. The nares and pharynx to be syringed twice a day.

July 26th.—Pulse 112. Looks brighter. Eye very much better. Union of flaps quite firm.

July 28th.—Improving. Discharges more moderate and less offensive. Upper jaw in good position, having fallen only a very little. Appetite good; bowels regular; sleeps well; no pain.

July 29th.—A small swelling of palate just behind incisor teeth

lanced. Discharge of pure blood. General condition of patient as good as usual.

July 30th.—Ligatures all away. External wound entirely healed.

Aug. 1st.—Quite comfortable. Discharge diminishing. A small piece of gutta-percha was moulded between upper and lower jaws of right side, and a bandage around the head and chin to keep the bone up in place.

Aug. 3d.—Steadily improving. Jaw in good position.

Aug. 5th.—Discharge from right nostril about normal. Patient walks about Hospital grounds, without suffering any inconvenience or pain.

Aug. 12th.—Still doing admirably. *No purulent discharge.* Some pain on pressure where the jaw was sawed across. Plug between jaws continued. No appreciable motion of parts of bone.

Aug. 22d.—Progressing very favorably. Bandage and gutta percha removed. Union of maxilla firm. Four weeks since operation.

Aug. 28th.—Discharged at his own request, well. Still wearing the wire about the teeth.

Sept. 2d.—Reported himself at the Hospital, in excellent condition. The wire removed. Union perfect; able to chew; now six weeks since the operation. Respiration clear. Examined with the rhinoscope by Dr. Langmaid, and the pharynx found healthy. A slight catarrh from the right nostril; nothing more.

ON THE TREATMENT OF PERMANENT URETHRAL STRICTURE.

By F. B. GREENOUGH, M.D., Boston, Surgeon of the Venereal Department of the Boston Dispensary.

[Communicated for the Boston Medical and Surgical Journal.]

THE frequency of urethral stricture and the great obstinacy with which it yields to curative means, have caused the subject of its pathology and treatment to occupy the attention of the most eminent surgeons; and many are the means that have been tried in order to conquer this very common ailment. The object of the writer of this brief article is to describe the simple method by which M. Maisonneuve, of the Hôtel Dieu, claims to have obtained many radical cures; but perhaps before giving an account of his operative procedure and instrument, a few general considerations may not be out of place.

Until recently, investigations on this subject have been carried on, comparatively speaking, in the dark, as the anatomical situation of the lesion is such as to render its study impossible except on the autopsy table, and this only showed that in a certain number of cases a more or less complete contraction of the walls of the urethra was found, caused by the presence of a dense fibrous tissue, generally in, or close to, the membranous portion, of the same gross appearance

and microscopic structure as the cicatricial tissue which we see on the common integument as the result of the healing of any lesion of continuity. But as to what the causes might be that produced this tissue, or the manner of its production, *post-mortem* examinations told nothing.

It being a characteristic of the human mind, and of the medical mind especially, to give a hypothetical reason for any existing fact, the causes of which are unknown, many hypotheses were advanced to account for the formation of strictures. The most natural supposition was, that, as on those parts of the body that are exposed to sight we see this, or similar, tissue formed as the result of some traumatic injury, such should also be the case in the urethra; and, indeed, instances were found in which there seemed every reason to believe that such was the fact. Contractions of the urethra were observed where there was a clear history of some previous injury, such as external violence, formation of false passages by catheterization, blows on the penis when in a state of chordee, &c. &c. But, on the other hand, in by far the greatest number of cases no such history could be obtained. It was, however, noticed that almost all such patients either were, or had been, suffering from a chronic gonorrhœa.

This fact was found to be so constant that the conclusion of cause and effect seemed to be justified; and the next step was to discover in what way the gleet could produce the stricture. The theory that it was the result of the use of strong injections found many supporters. This theory was the result of the "*post hoc, propter hoc*" process of reasoning; as it was proved that most patients who had stricture had used such injections. As, however, Ricord, by the same logic, proved that flax-seed tea was the cause of stricture, and as, moreover, we see that nitrate of silver, even in the solid stick, can be used on a mucous membrane with great freedom without causing a cicatrix, this supposition cannot be sustained. The explanation which has been most generally adopted, and which is found in most works on the subject, is in accordance with the pathological ideas of the day, and is, in a few words, as follows:—The chronic irritation of the mucous membrane of the urethra from a gleet produces an exudation of plastic lymph into the mucous and submucous tissues. This lymph becomes organized and forms a dense fibrous tissue, which contracts in the same manner that a cicatrix does, and thus produces the obstruction or stricture.

Virchow, when he revolutionized pathology with his cellular theory, showed that this explanation of the genesis of stricture was not correct. He has proved that no organized tissue can be formed from an exudation, but that all pathological as well as physiological tissues must spring directly from some tissue already existing, and that this is accomplished by means of a proliferation of the cellular elements (*zellenwukerung*). The result of his investigations has been to show that a tissue which heretofore has been known as merely

covering and binding together the various elements and organs of the body, plays a most important part in many pathological processes. This tissue is known as connective tissue (*bindegewebe*), and the proliferation of its cellular elements is the origin of many pathological processes and new formations (*bindegewebeswukerung*).

Such is the structure of the urethral stricture; it is due to a fibrous tissue, resulting from the hypertrophy of the physiological connective tissue of the urethra, which fibrous tissue has a strong tendency to contract, and thus to diminish the calibre of the canal.

It is, however, to M. Desormeaux that we are indebted for the discovery and demonstration of the manner in which this increase of connective tissue is produced by a gonorrhœal urethritis. He has shown by means of his endoscope that between the stage of acute urethritis and a fibrous stricture there always exists a granular condition of the urethra, which he calls "*urethrite granuleuse*." During this stage of the disease, cellulo-vascular granulations can be seen in the urethra in the membranous portion, or just anterior to it. These granulations are of the same character as those which we see at the base of any healthy wound or ulcer in the process of healing, and, like these, gradually change into fibrous tissue, which contracts.

We may, then, consider that a permanent urethral stricture is due to the existence of a contracting fibrous cicatrix, caused by the healing of an ulcerated condition of the mucous membrane, which ulcerated condition is the result of a chronic urethritis.

The symptoms of stricture are too numerous and well known to be recapitulated here, but a few words with regard to some of them may not be misplaced.

In all works on stricture great stress is laid on the size and character of the stream of urine as a means of diagnosis. As the size of the stream depends on the calibre of the urethra, and as this varies considerably in different cases, it alone cannot be of much value as a diagnostic sign.

A forked or twisted jet of urine is given in most books as one of the reliable symptoms of stricture, and the student is left to infer that this peculiarity of the stream is in some unknown way due to the obstruction. It is undoubtedly true that patients with stricture will often, especially on first rising, pass a double stream, or even one consisting of three or more jets, like that issuing from the nozzle of a watering pot; this, however, is not caused by stricture (at least not directly), but by the coagulation of the gleety discharge on being exposed to the air, which glues the lips of the meatus urinarius together at one or more points, and thus breaks the stream. As by the force of the flow of urine these adhesions are broken, the forked character of the stream ceases, and the patient passes a single jet. The twisted, or, as it has been called, the corkscrew jet, is not dependent on, or connected with, the stricture in any way, but is the normal form of the jet of urine. Professor Zeissl, of Vienna, ex-

plains this form of the jet of urine as follows. The shape of the meatus during the act of micturition is not like a simple slit, but more like an 8, in which the top and bottom have been drawn away from each other—or rather like an ellipse, the sides of which have been pressed in until they almost touch each other; so that the stream may be considered as consisting of two jets, one issuing from the superior and one from the inferior part of the meatus, being connected by a thin sheet of fluid which issues from the middle part. It is to the fact of these two jets crossing each other that the rotation of the stream is due. The upper jet falls upon the lower one, and by its force gives a rotary motion to the whole stream. That this is the case may be easily shown by slightly twisting the penis on its axis, when it will be noticed that the stream will change its twist from left to right, or from right to left, as the upper jet happens to fall on the right or left side of the lower one. Another characteristic laid down in the books as belonging to stricture is, that the last drops of urine dribble out and fall perpendicularly, instead of being projected in the same direction as the rest of the stream. This takes place, but in a less degree, in a normal stream, as, when the bladder ceases to contract and the internal vesical sphincter closes, whatever urine remains in the urethra is passed out merely by the action of the acceleratores urinæ and the compressor urethræ. The reason that this is most marked when there is an urethral stricture is, that the obstruction preventing the free passage of the urine through the urethra, the portion of the canal posterior to the obstruction becomes distended, and consequently when the sphincter closes there is more urine left in the urethra to dribble out. This dribbling of the urine may also depend on a partial paralysis of the muscular coat of the bladder. In short, it may be stated that the only *sure* proof of the existence of a stricture is that given by an exploration with a bougie.

With regard to the treatment of strictures, the indication is clear. The cause of the trouble being an obstruction in the urethra, the indication is to remove it; but unluckily, although the indication is clear, the means by which it is to be carried out are not.

As we know of no means by which the tissue forming the obstruction can be made to disappear and be re-absorbed, the next best thing that we can do is to force a passage through it, or, more correctly speaking, increase in size the small passage that exists; and this has been the aim of the various methods of treatment that have been adopted. These methods are as follows:—gradual dilatation, sudden and forcible dilatation, and incision.

The method by gradual dilatation consists in the daily passage of bougies, gradually increasing in size until the obstruction is fully dilated. This method is safe and not painful, and the only objections to it are, that it cannot always be done, and that the stricture is sure to contract again if the passage of a bougie is omitted for any length of time. The fibrous tissue in some chronic cases is so

firm and unyielding that even the most gradual increase in the size of the bougie will prevent its passage. In such cases we must resort to incision or forcible dilatation. The latter method has been strongly recommended; the best instrument is one which is known as Holt's dilator. This consists of a staff formed by two blades, which are united at one extremity, and between which there is a small rod. The staff is introduced into the stricture, and a tube is then put on the central rod and passed down between the two blades; as it is pushed down it separates the blades from each other, and thus forcibly ruptures the contraction. The objections to this method are, that it is sometimes very difficult if not impossible to get the stiff and unyielding staff through the obstruction; that if its point is made small enough to pass through a tightly contracted stricture, there will be considerable danger of forming a false passage in endeavoring to get it in, and that we have no means of limiting or even determining the amount of laceration of the tissues produced. Moreover, many strictures do not implicate the whole circumference of the urethra, but are confined to one of the sides, while the other remains covered with normal mucous membrane; in such cases the result of forcible dilatation would be, that that part would give way under the pressure that possessed the least power of resistance, i. e., the healthy mucous membrane rather than the tough fibrous tissue, and as this rupture must heal by the formation of cicatricial tissue, we shall have substituted in the place of a stricture occupying one side of the urethra, an annular one extending all around the canal. It remains to consider the method by incision.

From what has been already said, it must be evident that there can be, strictly speaking, no such thing as a radical cure of stricture, for inasmuch as the trouble consists in the existence of a fibrous contracting tissue which we cannot cause to be re-absorbed, and as, whether we break through this tissue, eat through it with caustics, or cut through it, there must always be a lesion of continuity, which in healing must produce new tissue of the same character, we have merely succeeded in obtaining a temporary freedom of passage, the tendency of which is to contract again and become as bad as ever.

This can, however, be prevented by the use of bougies, and it is to the judicious after-treatment by this means, much more than to the operative means used, that the successful treatment of stricture is due.

There are, however, some points to be considered in selecting the method of operation, and in the choice of an instrument. The operation, as performed by M. Maisonneuve, combines the advantages of simplicity, facility of execution, protection from haemorrhage and urinary infiltration, and is not very painful. The instrument he uses is an urethrotome invented by himself. It consists of a staff, grooved on its concave side, with a screw at its smallest extremity, a capillary elastic bougie which screws on to the staff, and a small triangular blade, which, by means of two small slots, fits into the groove of the

staff, and can be moved up and down. This blade is the shape of an isosceles triangle, with an obtuse apex; the apex is blunt, the two equal sides are sharp, and the base runs in the groove of the staff. The capillary bougie is first passed through the obstruction; the staff is then screwed on to it and passed down, the bougie going before it and as it were leading the way, and as it enters the bladder coiling itself up. When the staff is well through the stricture, the blade is fitted into the groove and pushed down; its blunt apex passes along the superior wall of the urethra, which is thus kept from the sharp edges, but as soon as the stricture is reached, the sharp side of the blade presses against it, and, guided by the staff, cuts its way through the obstruction. The whole instrument is then withdrawn, and a large-sized catheter is introduced and kept in for twenty-four to forty-eight hours. Such is the simple method of urethrotomy for which we are indebted to M. Maisonneuve. Its advantages consist in the use of the capillary bougie, by means of which we are enabled to pass the staff through the stricture without any danger of forming a false passage, and in the power which we have of regulating the depth of the incision; this depends on the altitude of the triangular blade used. Each instrument has three blades of different sizes. The introduction of the large catheter prevents the possibility of any haemorrhage by its pressure against the cut surfaces, and also protects the wound from contact with urine, until it is sufficiently glazed over with lymph to prevent the possibility of urinary infiltration. Unfortunately, M. Maisonneuve is not content with having originated the best known operation for incising an urethral stricture, but claims that by this operation he effects a radical cure. That this claim cannot be admitted is, it is hoped, sufficiently shown by what has been previously said. The cure, so far from being a radical one, is only just begun. The urethra must be kept dilated by passing bougies, at first daily, and then gradually decreasing in frequency, so that by the end of the third month once a week will be sufficient. The patient should be taught to pass a bougie on himself, and it should be plainly stated to him that for some years, if not forever, the neglect of so doing will expose him to a return of his trouble. Fortunately, the necessity of occasionally passing a bougie is not a great hardship, certainly nothing in comparison with the amount of suffering caused by a tight stricture.

In conclusion, it may be stated that gradual dilatation should always be tried, and urethrotomy be resorted to only when the stricture will not yield to this method. The remarks with regard to the after-treatment are equally applicable to all operations for the cure of stricture, from the simple gradual dilatation to perineal section.

DACRYOCYSTITIS.

[Communicated for the Boston Medical and Surgical Journal, by W. W. GARDNER, M.D. of Springfield, Mass.]

A YEAR ago some facts and queries concerning chronic Dacryocystitis were stated by me, in the *Medical and Surgical Reporter*, published in Philadelphia. Since that time my views have been somewhat modified, and as no response has been made by the profession I beg leave to call attention to the subject through your Journal.

Having several incurables (?) on my hands, then as now, who had followed treatment so long as to make me believe it a failure, I desired to know whether the fault was inherent in the theory or in me. Although one or two of the "incurables" have since assuredly recovered, by this persistence, and a few others improved so as to warrant a hope for success, I yet have other cases that time even fails to conquer. It is true that the majority of cases easily recover by the plan pursued in a short time, but the obstinate cases are opprobria it would be delightful to wipe out at an early day.

My plan is similar to that in our Eye Hospitals, to slit the canaliculus about one half the distance from the puncture to the sac, and for a few days during each day following, keep the slit open so that it will be permanent. Next—to use the largest size Anel's probe, which can pass the stricture in the nasal duct, and repeat once in from four to ten days, using a larger probe as often as possible until No. 7 is used.

I seldom use No. 8 probe, because my successful cases get well with No. 7, and my unsuccessful ones continue, although I use No. 8. If there is much accumulation in the sac, I use Anel's syringe with a weak solution of chloride of sodium, to wash it out, each time after using the probe.

My theory is, that after use of the probe there will probably be a little blood which coagulates and stops the passage, and that the retention of mucus or pus until the coagulum is broken up, or until the next probing, prolongs the case. If pure water is used to cleanse the passage, it will corrugate the mucous membrane, or by endosmosis thicken it, so as to in a measure reclose the duct for a time, and hence the sodium is indicated. To demonstrate this by analogy, try first water, then chloride of sodium solution with the nasal douche, and the effect will be convincingly apparent.

I have tried the nasal douche, as adjunctive treatment the past year, and from my observation believe it to be very useful in many cases.

The constitutional treatment is as essential as in catarrh, in a majority of cases, but has no specific relation to the disease. The patient is treated. If there is debility, vegetable and ferruginous tonics are advised, and alteratives if complicated with scrofulous or strumous habits.

The most obstinate cases are those tending to erysipelatous inflammation, and some of these could not bear the use of the probe often enough to be useful without the use of a cholagogue cathartic once in ten or fifteen days. Some who have recovered so far that the probe has not been needed during six or eight months past, are obliged, occasionally, to take the cathartic once in six weeks to prevent the inflammation recurring in the sac. My prescription has, usually, been podophillin gr. one-fourth at night—repeated until it moves the bowels.

One case, a lady, who had fistula lachrymalis upon one side, and who had often been confined to her room because of the acute attacks of inflammation of the sac, was under my care two years, the probe having been used over a hundred and fifty times. The bone surrounding portions of the sac was denuded of membrane, and the natural course of the duct evidently altered. At no time, during her treatment, could the probe be passed quite into the nasal fossa without using improper force, although its shape was changed to meet any alterations there might be.

During her treatment she had no acute attacks of inflammation of the sac, and aside from the trouble of treatment, no inconvenience; but after so long efforts, it seemed useless to continue the probes, and after instructing her how to wash out the sac she was discharged. Nearly a year intervened before she returned, during which time she constantly cleansed the sac with the syringe, and had no other trouble with the disease. Another trial of the probes was suggested, to which she assented. A No. 7 probe, bent at the point unusually backwards, readily passed into the nasal fossa, and was repeated twice afterwards within two weeks, and since then, over a year in time, she has had no vestige of the disease.

I am well satisfied that the primary operation will *sometimes* secure a recovery, its advantages are so apparent. Whether it is because of the stimulus of the probe, the slight local depletion from the operation, or because the canal is enlarged, or all together, I am not sure, but think all three influences work in our favor.

Patients from a distance generally desire to hurry matters, and I have sometimes passed the probe daily a few times, but I cannot think much is gained, as the irritation has to abate before repeating the probing.

Those cases presenting only a watery eye, without muco-purulent secretion, generally get well after using the probes from five to ten times. Those recover the soonest who are in good health.

September, 1867.

THE Commissioners of Public Charities and Correction in New York are to erect a building on Ward's Island as an Inebriate Asylum, for the reclamation of drunkards.

VACCINATION.—RE-VACCINATION.

[Communicated for the Boston Medical and Surgical Journal.]

MESSRS. EDITORS,—Far from intending "an admonition" in the closing paragraph of my paper with the above title (JOURNAL, Aug. 29th), as assumed in your remarks in the JOURNAL of to-day (Sept. 15th), I only wished to deprecate insinuations of ignorance and neglect against those who honestly hold different opinions, and openly state the grounds on which their opinions are based. My paper was founded on independent observations and facts; but as it evidently seems to have been misapprehended, I ask your permission to briefly re-state its substance.

The test, a true one, being long-continued exposure to variolous disease—in my cases,

Some having large foveated and foveolated scars were shown to be imperfectly protected;

Some having only "a miserable speck or two" proved to be amply protected;

So that scars alone, whatever their characteristics, so far as these cases go, cannot be relied on as tests of protection. And further,

Some, thoroughly, and about a quarter of the whole number so young that they may be said to have been recently vaccinated, were attacked with varioloid;

Some escaped, though many years had elapsed since vaccination;

Youth did not give, age did not take away immunity.

Such were the facts. Few as they were, being facts, they must stand.

As to re-vaccination, it is clearly evident that the only way to satisfy all the conditions exacted by the several parties of advocates, except such as are named in my paper, will be to repeat the operation every few years, nay, perhaps every few months—thus reducing the whole proposition to an impracticable absurdity.

Lastly, I have daily more and more reason to believe that the damage arising from indiscriminate re-vaccination, even when done by those claiming to be preëminently successful, has not been overestimated in my paper.

Very respectfully yours,

Roxbury, Sept. 19, 1867.

B. E. COTTING.

CONTRIBUTIONS TO DERMATOLOGY.

[Continued from page 37.]

Treatment of Impetigo in Children.

If the patient is a nursing child, the milk should be examined under the microscope, if practicable, and if found at fault, the child should receive its supplies of good nourishment from other sources. If it is old enough to be weaned, this change should be made rather than to

procure a new wet-nurse for it, for in the latter event great risk is run, that the blood and therefore the milk of this nurse is of impure quality. The child will thrive well on good milk from the cow, or a little cream added to the milk occasionally; strong beef tea now and then, will also be proper, care being taken to watch its effect on the bowels; or it may be allowed to suck a bit of lean beef two or three times a day. The above will afford a generous bill of fare from which a judicious mother can make a selection that will keep her offspring in a healthy state, so far as diet is concerned. Gingerbread, and all kinds of cake are to be avoided, for they will soon take from the child the power to digest the simplest food. Having suitably regulated a scale of dietetics, and made all needful provision to secure salutary hygienic influences, the way is prepared for general and local treatment. In regard to the former, but little is required for a young child.

If the disease is on the head, it will be proper to administer a laxative of the submuriate of mercury, one grain rubbed up with the same quantity of loaf sugar, and dropped upon the tongue of a child from one to two years old. This may be repeated every eight or ten days to a child of good general health. If it is of a feeble frame and condition, then the calomel is contra-indicated. Tonics are called for instead. The syrup of the iodide of iron and Fowler's solution of arsenic, as recommended in the treatment of eczema infantile in a previous paper, are equally indicated in cases of infantile impetigo. And these are about all the constitutional remedies that are applicable to a young child from one year to eight or ten years old.

The local treatment should now engage our attention. The scabs formed by the contents of the pydracious pustules are easily softened and made ready for removal by the application of a solution of the carbonate of soda; when the scalp is the seat of disease, **R** Soda carbonatis, 3*i.*; aquae fontanæ, 3*viii.* M. Soft rags saturated with this solution are to be constantly applied to the diseased integument, and an oiled silk cap should be placed over the rags to prevent evaporation. As soon as the scabs are removed, the raw surface should be wiped dry and carefully examined to ascertain if any small ulcerated spots have formed, and if so, they should be coated over with a weak oxide of mercury ointment, 3*i.* to 3*i.* If any little abscesses are detected, they should be punctured with the point of a lancet. They soon heal up without further trouble. If there is much hair on the scalp, it should be clipped short and kept so.

After the scabs are removed, a solution of the carbonate of soda, 3*i.* to 3*viii.* of water, will make a suitable dressing to the scalp. Cloth saturated with this should be laid on smoothly, so that the lotion shall come in contact with the entire surface. The pruritus, which is generally a characteristic of the eruption in its early stages, is in most instances allayed by this local remedy, which is to be em-

ployed so long as its efficacy is apparent. If it proves inadequate to the exigencies of the case, the following will be found an excellent adjvant. **R.** Acidi hydrocyanici diluti, 3*i.*; spiritus vini, 3*ss.*; emulsionis amygdaleæ, 3*viii.*; **M.** Signa.—Lotion. Lint saturated with this is to be applied for an hour or two each day, to the affected parts; and during the rest of the time the alkaline solution is to be employed. If the sedative effect of the hydrocyanic acid is not sufficiently powerful, it can remain on for a longer time than is mentioned, and can be repeated with safety morning and evening.

The benzoated oxide of zinc ointment is also an appropriate local application at this crisis, especially by night. It should be laid on so as to form a thick coating to the scalp, and a night cap should be fitted to the head.

A mixture containing the oil of cade, rectified spirit, and a small quantity of caustic potash, and used as a local application, is also beneficial when the itching and irritation in the parts are obstinate. **R.** Potassæ fuse, grs. xv.; olei cadini, 3*ss.*; olei camphorati, 3*i.*; spiritus vini, 3*ii.* **M.** A portion of this is to be rubbed briskly over the eruption night and morning, and to be washed off with warm water and soap before each re-application. As the pruritis abates, the lotion may be used less frequently; and during the intervals of its application the scalp is to be covered with the benzoated zinc ointment.

After the severity of the local symptoms has materially subsided and the eruption has passed into a chronic state, an ointment of the nitric oxide of mercury will be found appropriate. **R.** Hydrargyri oxidi rubri, grs. xxiv.; pulv. camphoræ, grs. vi.; unguenti simplicis, 3*ii.* **M.** This ointment is to be applied freely two or three times a day to the affected surface. In a majority of cases of impetigo capititis the foregoing method of treatment will be found efficacious, and sufficient to cure the complaint.

[To be continued.]

Reports of Medical Societies.

EXTRACTS FROM THE RECORDS OF THE BOSTON SOCIETY FOR MEDICAL IMPROVEMENT. BY CHARLES D. HOMANS, M.D., SECRETARY.

AUG. 26th.—*Tuphlo-enteritis-peritonitis after an Accident, with Specimen.*—Dr. C. D. Homans reported the case.

A girl, eight years old, of slender constitution, was thrown from a child's truck on which she was being dragged, on Wednesday, falling upon her right side. On Thursday she had a chill, followed by pain in the right iliac region, which gradually extended over the whole abdomen. She was under the care of an irregular practitioner till Sunday, when Dr. Homans was called, and found her in bed, countenance anxious, pulse frequent and weak, complaining of great pain and tenderness in bowels, just over the situation of the cœcum, and

with great anorexia; the bowels were constipated; there was some tympanitis and tenderness generally over the abdomen. By stimulants and opiates she was made as comfortable as possible, but gradually sank away until her death, on Monday afternoon.

The autopsy was made by Dr. J. HOMANS, Jr., who showed the specimen and furnished the following account:—

Abdomen only examined. Recent peritonitis, with effusion of purulent fluid, and gluing together of the folds of intestine and contents of the pelvis was found. The inflammation was most severe in the neighborhood of the cæcum. There were two small perforations of the appendix cœci, seven eighths of an inch from its extremity, round and small, perhaps half a line in diameter. The last inch of the appendix was in a more or less advanced state of gangrene. The tissue of the appendix, at its circumference where the perforations were found, was reduced to a thin covering of peritoneal and areolar tissue. Within the appendix—two inches from its origin—was found, pretty firmly fixed, a body of a brown color, faecal odor, and of the size and shape of an ordinary dry, white bean. This body was three and a half lines long, by two lines in thickness, and was of the consistency of moderately hardened putty, or very thick, tough dough. On section, it was seen to have a small central point of a bluish-slate color (this point is soft), and deposits seem to be arranged concentrically around this centre.

By the aid of the microscope, this body is seen to consist of portions of vegetable structures and amorphous matter, such as might be seen in faeces.

Dr. JACKSON remarked upon the fact that acute disease not infrequently follows an accident, a point not sufficiently insisted upon by authors; he thought the child would have been alive now but for the fall.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON: THURSDAY, SEPTEMBER 26, 1867.

METHODS OF CALCULATING THE EPOCH OF PARTURITION FROM THE LAST PRECEDING MENSTRUATION OF THE PREGNANT WOMAN.

In the to us indispensable pocket memorandum book, called the "Physician's Visiting List," is a table for calculating the period of utero-gestation, as it is called. According to this, confinement may be expected, *at the soonest*, on the expiration of nine calendar months from the *end* of the last menstruation; and *at the latest*, at the termination of ten *lunar* months from the close of that last menstruation; the interval varying (on account of the different length of the calendar months included) from four to seven days.

That this is cutting a Gordian knot too abruptly, it is easy to show. But, we have chosen our present topic because we think it is not always sufficiently borne in mind how much variation there is in the normal duration of pregnancy, and that to fix the precise day or week of approaching delivery is attempting too much.

This fact is foreshadowed, perhaps, by the different modes in use of computing the time when confinement may be expected. The subject seems to have been rendered somewhat more obscure by want of precision in fixing the date from which the calculation is to be made. Thus, some date from the end of the last menstruation, others from the beginning of it. In the statements, too, of authors, it is not always clear whether they mean the interval between actual conception and labor, or the average time which elapses between the last menstruation (whether the beginning or the end of it) and parturition. As patients are not unfrequently subjected to a good deal of annoyance from disappointments in their calculations, either by delay in the arrival of their expected delivery, or by its setting in before it was anticipated, we think a brief review of the question may not be amiss, in order that we may fully realize how the matter stands, and guard against the inconveniences just alluded to.

The old-time method of calculating the date when the event in question should occur is to count nine months and a half from the last menstruation. We believe it is the close of that menstruation, too, that is taken for the point of departure.

But, the illustrious Harvey declares that pregnant women "after ten lunar months have elapsed [from the last menstruation] fall in labor, and reap the fruit of their womb, the very day on which the catamenia would have occurred had impregnation not taken place." He also says that matrons calculating after this rule "are rarely out of their reckoning."

Dr. Tyler Smith also argues that parturition is to be expected at what would otherwise be a menstrual period (and that the tenth); and also that the process is essentially a menstrual period, on the ground that the duration of pregnancy (280 days) is the multiple of a lunar month.

Dr. Smith says on this subject:—"I believe I have proved that *ovarian excitement is the law of parturition in all its forms of ovi-expulsion.*" He subsequently makes the quotation from Harvey which we have given above. Dr. Smith remarks, "this, it will be seen at a glance, is merely the expression of a numerical fact," and urges his claim to have originated the idea that ovarian excitement produces parturition as well as menstruation. His reference to the 280 days of pregnancy as being a multiple of a catamenial period is a part, also, of his favorite theory of "the great genesial cycle." In describing that theory, he says:—"Thus the catamenial cycle of twenty-eight days is departed from at conception for another cycle—namely, that of gestation, which consists of 280 days, or ten lesser cycles. After the completion of gestation a new cycle is commenced—that of lactation—upon the completion of which the system returns to the simple catamenial cycle. These cyclical and epicyclical periods are themselves all included in another great period of development extending from puberty to the decline of the catamenia."

If Dr. Tyler Smith really means to prove that labor falls on a menstrual epoch because the period of *gestation* is a multiple of a catamenial period, then, as Dr. Montgomery has well shown, he involves himself in a *non-sequitur*: For the commencement of pregnancy, or conception, occurs not during menstruation (save by exception), but usually during the week following that function, and is not limited strictly, as we shall take occasion to show, to any portion of the intermenstrual period. It may be, however, that this writer has embarrassed himself by the confusion of terms we have above referred to, and may, in fact, mean by

the duration (or "cycle") of gestation the interval from the last menstruation of a pregnant woman to her confinement, assuming that to be ten lunar months. In that case, his theory may be consistent with the alleged numerical facts, and then the question turns upon the validity of those facts. As to that, we will say in advance, that, as we shall presently show, labor can be said to occur at the end of the tenth lunar month from the last menstruation only as a general average—a mean result between wide extremes.

Now, thus far we have found that different authorities have given us different data for calculating the time of expected confinement—ten lunar months from the last menstruation of the pregnant woman is one rule; nine calendar months and a half from the said last menstruation is another, &c. Dr. Churchill thinks even this latter mode of calculation is in the majority of cases *over-run*.

But we have to go a step further, and show that the duration of pregnancy itself, comprehending, that is, the interval between *conception* and the delivery of a mature fetus, admits of not merely exceptional, but frequent variation. This appears, in the first place, from the fact that the French authors Jacquemier and Cazeaux set the average term of gestation at 270 days, while English writers often put it at 280 days. The same thing is more distinctly shown in a table from "Montgomery on Pregnancy." Montgomery takes Reid's table of "25 cases dating from a single coitus," and a table of his own, consisting of "56 cases in which the day of fruitful intercourse was known," and presents some of the combined results in a tabular form. From this last table it appears that of the 81 cases of Reid and Montgomery together there occurred

In the 35th week	1 case.
" " 37th "	2 cases.
" " 38th "	6 "
" " 39th "	15 "
" " 40th "	36 "
" " 41st "	10 "
" " 42d "	9 "
" " 43d "	2 "
Total	81 "

The above figures, we find on examination, show that there were more than twice as many cases of mature delivery on the 40th week as in any other *one* week; but yet that there were only 36 cases in the 40th week to 45 cases in the other weeks taken together—24 cases in the weeks preceding the 40th, and 21 in those succeeding.

This result, we take it, demonstrates precisely what we have undertaken to show, viz., that there is great variation in the duration of pregnancy, and that a wide margin should be left for such variation in arranging for an expected confinement. The single case in the 35th week might be set aside as an extreme exception without affecting the results materially.

We have taken the trouble to further analyze the tables of Reid and Montgomery so as to get the average duration of gestation in *days*. The total number of days in Reid's 25 cases we find to be 6852. Dividing this number by 25, we strike an average of 274 $\frac{1}{2}$ days. In the 25th case there was sexual congress on the 15th, 16th and 17th of September, labor occurring July 5th. We have adopted, in our calculation, the intermediate day (the 16th), and assigned 292 days as the duration of gestation in that case. We have been obliged to act in

a similar way in 7 of the 56 cases of Montgomery, which we now proceed to analyze in the same manner as we have just done with Reid's.

The total number of days in Montgomery's 56 cases is 15,589. Dividing this number by 56, we get an average of 278 days.

Once more: the total number of days in the 81 cases of the two tables combined (25 of Reid's and 56 of Montgomery's) is 22,441 (6852 + 15,589). Dividing this by 81, we have an average of 277 $\frac{1}{3}$. But, the French authors we have quoted make the average duration of pregnancy 270 days. And, the mean between the French average of 270 days and the English average of 277 days is 273 $\frac{1}{4}$, which brings us back to *about* the average of Dr. Reid's cases—viz., 274 days.

Now, let us see if we can infer the average time which elapses between the day on which the last menstruation of a pregnant woman is recorded to have commenced and labor; 273 $\frac{1}{4}$ days we have seen to be (so far as the statistics we have analyzed go) the average duration from *conception* to labor. Conception usually occurs during the first seven or eight days after menstruation. These are our data. It follows from them, that the average we have above obtained of 273 $\frac{1}{4}$ days *plus* the time occupied by the menstrual period will give us *from forty to forty-one weeks* (according, perhaps, as conception occurs on first, or the eighth or an intermediate day) as the average time which intervenes between the beginning of the last menstrual period a pregnant woman has had and labor.

As forty weeks are ten lunar months, we have approximated to the aphorism of Harvey (adopted by Tyler Smith and others) that the pregnant woman "shall fall in labor" on the completion of the tenth lunar month from her last menstruation. Only we find that Harvey was in error in saying that variations from this rule were rare. For, we repeat that the divergences are many in number and considerable in degree.

In the same way, the rule given by the "Physician's Visiting List," alluded to at the beginning of this article, telling us to count nine calendar months from the close of the last menses as the soonest, and ten lunar months as the latest time to expect labor, corresponds nearly enough to our 40 to 41 weeks from the commencement of the last menstruation. But here, again, the error is that allowance is not made for divergences.

In practice, our patients should be prepared (if the statistics we have interrogated are a safe guide) considerably before the calculated time, and yet not be disappointed if they should go two weeks, or even longer, beyond it. Thus, to suppose a case, a woman begins to menstruate on the 1st of January and ceases on the 5th. Having no return of menstruation, she calculates to be confined in from 40 to 41 weeks from January 1st—i. e., from October 7th to October 14th. She conceived, we will assume, January 8th, three days after the cessation of the menses. If gestation in her case should last only 38 weeks, then labor sets in October 1st, or one week before she began to expect it. On the other hand, suppose the gestation prolonged to 42 weeks, the patient, instead of being confined by the 14th of October (i. e., from the 7th to the 14th, as before), goes to the 21st.

But, supposed prolongation of gestation may be sometimes more apparent than real. This appearance of prolongation may arise from the fact that conception is not always limited to the first eight days after menstruation. For example, in

Dr. Reid's 25 cases "dating from a single coitus" are three cases in which the single coitus, and consequently the conception, took place respectively 11, 12 and 16 days after the cessation of the menses. In all of those three cases labor took place within the 40th week after conception; in the first case on the 276th day, in the second case on the 275th day, in the third case on the 278th day. Yet in the two first of the three cases—those in which the conception occurred on the 11th and 12th days respectively after the close of menstruation—precisely 41 weeks elapsed from said close of menstruation to labor, and therefore more than 41 weeks from the commencement of that menstruation. In like manner, in the third case, where conception occurred on the 16th day after the close of menstruation, precisely 42 weeks elapsed from said close of menstruation to labor, and therefore more than 42 weeks from the commencement of that menstruation.

These instances of conception late in the inter-menstrual period do not materially impugn the "ovulation" theory of menstruation, because even if it be doubted that an ovum may retain its vitality beyond eight days after menstruation, yet we may fall back on the fact which, as Dr. Montgomery says, Dr. Ritchie has satisfactorily shown "by dissections of females made at all periods of life, that Graafian vesicles are ruptured and ovules extended even in children, and in women during the child-bearing period of life, at times apart from the menstrual period." From this it would appear that the *molimen hæmorrhagicum* of menstruation is only subsidiary to, or collateral with the more essential part of the function, viz., the oviposit.

Finally, the apparent protraction of gestation, owing to conception taking place just before an expected recurrence of menstruation which fails to appear, is too familiar a point to require more than this passing mention.

THE report in the *Union Médicale* of the proceedings of the International Medical Congress gives a highly graphic description of the sessions of the 23d and 26th of August.

The Congress, it seems, was pursuing the course of its labors in peace, and was even a little sleepy, when *syphilization* intervened, and behold a war broken out! The brand of discord was thrown into the midst of the pacific assembly. The reporter declares that he never before had been present at a congress where the sessions were more noisy and stormy. The audience, ardent, passionate, tumultuous, was divided into two camps; the one for, the other against syphilization. The latter, the more numerous, supported M. Ricord with loud acclaim. For Ricord, "these two sessions were, from beginning to end, a long ovation, and a continuous triumph. The illustrious syphiliographer may be said to have received on those memorable days the acclamations of the entire world, the crown of his splendid career." The other party, smaller in number, but with no less energy, sustained by voice and gesture M. Auzias-Turenne, who displayed in this unequal strife a talent, an activity, a firmness, and a warmth of conviction which it was impossible not to admire, though without sharing his opinions.

In the midst of the general excitement, a ludicrous incident occurred, to change for an instant the current of feeling. M. Ricord was saying that "he could desire nothing better than to believe in syphilization. From the outset of the alleged discovery of M. Auzias-Turenne he had said, if syphilization were a reali-

ty it would follow that the soft chancre was to the hard chancre what vaccinia is to variola. M. Auzias-Turenne would be the Jenner of syphilis, and would have statues erected to him. But, unfortunately, the vaccinia of syphilis has not yet been found, and the pox still awaits its Jenner. If M. Auzias-Turenne believed in syphilization, let him syphilize himself. Quite enough patients had purchased with their health—even with their lives—the pretended benefits of syphilization." At this moment, a member of the convention, getting up on one of the benches of the amphitheatre, and stretching out his arms, cries out twice, in a voice of thunder, "I am a physician—syphilized—and I am in good health." (Prolonged merriment.)

(A voice from the auditory.) "Why has not M. Auzias-Turenne syphilized himself?"

(The previous speaker.) "M. Auzias-Turenne has not renounced marriage. As for me, I have renounced it. What father, what mother would be willing to give a daughter to a syphilized man?" (Laughter.)

M. Ricord.—"Nay, if syphilization be a guarantee, syphilized subjects would be much sought for as family men." (Laughter.)

M. Auzias-Turenne.—"I have offered to make experiments before a commission; but my demand has always been rejected. It is desired that I make the experiments on my own person. I refuse from self-respect, not being willing thus to place myself at the disposal of M. Ricord and M. Bouillaud, for the sole purpose of gratifying their curiosity, and being afterwards the butt of their pleasantries."

In the midst of a warfare of words between M. Ricord and M. Auzias-Turenne, M. Crocq remarked that the pending discussion was widely divergent from the question of the day, which was this:—*Is it possible to propose to the different governments any effectual measures for checking the spread of venereal diseases?* A delegate from the Belgian Government, would it be acceptable, he asked, for him to propose to his government to syphilize all the Belgians in order to check the spread of syphilis in Belgium? (Laughter.) M. Crocq demanded that the discussion be closed. It was, however, continued through the succeeding session.

Subsequently, there was a prolonged debate upon legislative enactments having in view the eradication or diminution of venereal diseases. In the remarks on this question, it appeared that the registration and regulation of prostitution had been a failure even in Paris, where the registered are far outnumbered by the clandestine prostitutes.

VITAL STATISTICS OF BOSTON.
FOR THE WEEK ENDING SATURDAY, SEPTEMBER 21st, 1867.
DEATHS.

	Males.	Females.	Total.
Deaths during the week	47	34	81
Ave. mortality of corresponding weeks for ten years, 1856—1866	46.7	45.3	93.0
Average corrected to increased population	00	00	103.00
Deaths of persons above 90	0	0	0

DEATHS IN BOSTON for the week ending Saturday noon, Sept. 21st, 81. Males, 47—Females, 34. Accident, 2—colic, 1—bronchitis, 4—congestion of the brain, 2—disease of the brain, 1—cancer, 1—cholera infantum, 8—cholera morbus, 1—consumption, 14—convulsions, 5—diarrhoea, 1—diphtheria, 3—dropsy, 1—dropsy of the brain, 1—dysentery, 4—fever, 1—scarlet fever, 3—yellow fever, 1—disease of the heart, 2—disease of the liver, 2—congestion of the lungs, 1—marasmus, 3—old age, 1—peritonitis, 1—pleurisy, 4—premature birth, 1—puerperal disease, 1—sunstroke, 1—teething, 1—ovarian tumor, 1—unknown, 10.

Under 5 years of age, 37—between 5 and 20 years, 9—between 20 and 40 years, 14—between 40 and 60 years, 15—above 60 years, 6. Born in the United States, 60—Ireland, 18—other places, 3.